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**Juvenile Arrest and Collateral Educational
Damage in the Transition to Adulthood**

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Juvenile Arrest and Collateral Educational Damage in the Transition to Adulthood

ABSTRACT

Official sanctioning of students by the criminal justice system is a long-hypothesized source of educational disadvantage, but its explanatory status remains unresolved. Few studies of the educational consequences of a criminal record account for alternative explanations such as low self-control, lack of parental supervision, deviant peers, and neighborhood disadvantage. Moreover, virtually no research on the effect of a criminal record has examined the “black box” of mediating mechanisms or the consequence of arrest for postsecondary educational attainment. Analyzing longitudinal data with multiple and independent assessments of theoretically relevant domains, this paper estimates the direct effect of arrest on later high school dropout and college enrollment for adolescents with otherwise equivalent neighborhood, school, family, peer, and individual characteristics as well as similar frequency of criminal offending. We present evidence that arrest has a substantively large and robust impact on dropping out of high school among Chicago public school students. We also find a significant gap in four-year college enrollment between arrested and otherwise similar youth without a criminal record. We assess intervening mechanisms hypothesized to explain the process by which arrest disrupts the schooling process, and, in turn, produces collateral educational damage. The results imply that institutional responses and disruptions in students’ educational trajectories, rather than social psychological factors, are responsible for the arrest-education link.

School completion represents a critical marker in the transition to adulthood, perhaps now more than ever as stratification by education continues to increase. Because delays in education or its cessation altogether can significantly alter life trajectories related to work and family formation, it is critical to understand the sources of educational attainment.

The purported causes of educational disadvantage are many, but one long-hypothesized factor has received little rigorous attention—official sanctioning by the criminal justice system. Empirical inattention to the educational consequences of criminal justice sanctions for students is puzzling given the high prevalence of arrest among young Americans, particularly urban African American males (Hirschfield 2009; Kirk 2008; Western 2006). Indeed, 9 of every 100 male youth aged 10 to 17 are arrested annually in the United States (OJJDP 2009). In Chicago, as in many metropolitan areas, the rate is considerably higher, with a rate of 15 arrests per 100 male youths (or roughly 25,000 arrests each year) (Chicago Police Department 2006). One-quarter of these arrests occur in school.

There are several theoretical reasons to expect that official interventions such as juvenile arrest have consequences for educational attainment. Social control theory implies that weak bonds to school exacerbate problem behaviors such as truancy and school dropout. If the arrest of a student independently fosters alienation and weakened attachment to school, arrest may indirectly lead to dropping out. Rational choice theories suggest that students may drop out of school or opt not to enter college following arrest because they assess (perhaps correctly) that the touted benefits and added utility of education are not likely to materialize given the stigma of a criminal record. Dropout may follow arrest because of status frustration: a criminal record may make it harder for a student to compete against non-criminal schoolmates, with dropout serving as a solution to this status frustration (Elliott 1966; Elliott and Voss 1974).

Perhaps the most salient prediction comes from labeling theory, which asserts that being officially designated a “criminal” changes the way educational institutions treat students. In the interest of accountability and school safety, students with criminal records may be pushed out of high school through exclusionary policies, and they may be segregated into specialized programs for problem youths (Kirk and Sampson 2011). The stigma of a criminal label may also damage social relationships, thereby leading to rejection from teachers, parents, and pro-social students (Lemert 1951). Similarly, labeled offenders may face diminished prospects of enrolling in college because of unstated admission criteria as well as an inability to secure financial aid. Arrest may also reduce chances for high school graduation and college enrollment because time spent in court, in juvenile detention, or reporting to a probation officer leads to absences, a blemished transcript, and an unstable educational trajectory.

There are important counterarguments to these predictions, however. The most prominent is that arrest and educational attainment are spuriously correlated, with each explained by a third factor such as low self-control. Gottfredson and Hirschi (1987:601-602) are strong proponents of this view, arguing that the “apparent ‘effect’ [on future delinquency] of criminal justice processing is merely an artifact of ‘selection bias.’” In this view, external events such as an arrest do not influence dropout or other forms of future delinquency because delinquency is the product of a stable propensity established early in life. By failing to account for variation in self-control, researchers, Gottfredson and Hirschi (1987; 1990) argue, have erroneously concluded that criminal justice sanctions have labeling effects that produce continued delinquency. In addition, there are reasons to suggest that arrest may lead to a *decline* in the likelihood of dropout. Juvenile arrests that result in probation may carry a mandate of school

attendance to avoid conviction (Mayer 2005). Compulsory attendance of this form may thus potentially strengthen attachments to education and lessen the likelihood of dropout.

This paper aims to adjudicate among these competing hypotheses by examining whether and why juvenile arrest contributes to later school dropout and hinders the prospects of college enrollment. We argue that, compared with incarceration, arrest is more “random” or variable in the juvenile population and therefore offers increased analytic leverage in estimating causal effects of criminal sanctioning. Incarceration is the last step in criminal justice processing such that individuals who make it to prison are for the most part so unlike the general population that counterfactual comparisons are difficult to make (Loeffler 2011). Exploiting this relative difference in randomness, we use individual-level propensity score matching to answer three main research questions: (1) Does juvenile arrest increase the likelihood of later high school dropout? (2) If so, *why* does arrest hinder educational attainment among high school students? (3) Finally, does arrest independently diminish the likelihood of college enrollment among young adults?

CRIMINAL STRATIFICATION AND THE LIFE COURSE

In *Punishment and Inequality in America*, Bruce Western (2006) details the systemic consequences of “mass incarceration” and the prison boom of the 1990s and 2000s on social inequality, in particular as it relates to wages, employment, and family life. He notes that the risk of imprisonment is concentrated in one social group—black male high school dropouts. Roughly 60 percent of this segment of society can expect to spend time in prison by age 34 (Western 2006:27). But this is a lifetime prevalence estimate, and most adult prisoners have accumulated long criminal records that set them apart from their non-prison counterparts

(Blumstein et al. 1986), making it difficult to directly assess the unique experience of being in prison from confounding factors.

In our view, the stratifying mechanisms that sort lower-class blacks and Latinos from middle-class groups occur earlier than incarceration and, we argue, are embedded in educational failure as well as characteristics of neighborhood, school, family, and peer context. Although we do not deny the negative consequences that a term of imprisonment can have for one's life-course prospects, well before reaching an adult penitentiary most offenders have already traveled a path that includes juvenile crime, arrest, and ultimately educational failure. In this sense, prisons may be the warehouses for containing those segments of society already marginalized, in contrast to being the primary source of that marginalization (see also Wacquant 2000). If this is true, the causal effect of incarceration may be smaller than originally anticipated (Loeffler 2011). Our goal in this study is therefore to look directly at the stratifying consequences of events that come earlier in the life course than incarceration. Again, it is not that imprisonment is inconsequential, but rather that the story of punishment's salient role in shaping inequality in America should begin with an investigation of the potential "turning-point" consequences of juvenile arrest.

A large and convincing body of research dating back to the 1940s documents that a majority of adolescents engage in some form of delinquent behavior (e.g., Porterfield 1943; Short and Nye 1957; 1958; Wallerstein and Wyle 1947), with rates of delinquency peaking in late adolescence and declining precipitously thereafter (Gottfredson and Hirschi 1990; Sampson and Laub 1993). An equally compelling body of research reveals that only a small proportion of all delinquent acts come to the attention of the police, and of those that do, only a small proportion result in arrest. In their classic study, for example, Black and Reiss (1970) found that

only 15 percent of police contacts with juveniles resulted in an arrest, providing evidence of considerable discretion on the part of police. Although arrest in theory requires that a crime was committed, most criminal incidents do not end in arrest. Akin to the paradox identified by Lee Robins (1978) whereby most teenage delinquents do not become adult criminals even though virtually all adult criminals were juvenile delinquents, virtually all arrestees have committed crimes but far from all those committing crimes are arrested. The commonality of delinquency combined with the stochastic nature of arrest in the adolescent life course has potentially major consequences.

Life-course theories of cumulative disadvantage provide an orienting framework for analyzing the consequences of early contact with the criminal justice system and the remarkable continuity in problem behavior over the life course. Sampson and Laub's (1993; 1997) theory of sanctions is derived from a linkage of social control (Hirschi 1969) and labeling (Becker 1963; Lemert 1951) theories with life-course principles about stability and change. A key hypothesis is that once an individual is labeled a deviant (e.g., through an arrest record), a variety of detachment processes are set in motion that promote the likelihood of further deviance, including school dropout, and lessen an individual's likelihood of a successful transition to adulthood. Sampson and Laub (1997) thus argue that official sanctions serve as a negative turning point:

Cumulative disadvantage is generated most explicitly by the negative structural consequences of criminal offending and official sanctions for life chances. The theory specifically suggests a 'snowball' effect—that adolescent delinquency and its negative consequences (e.g., arrest, official labeling, incarceration) increasingly 'mortgage' one's future, especially later life chances molded by schooling and employment. (P.147)

Similarly, Moffitt (1993:684) describes how some delinquents become "ensnared" by the consequences of their antisocial behavior, thereby narrowing the opportunities available to them to follow a prosocial behavioral repertoire. The snare of arrest may be an irrevocable event that drastically curtails a delinquent's opportunity to "go straight."

Mechanisms Leading to High School Dropout

How exactly would the stigma of an arrest record hinder the likelihood of high school graduation? One potential answer is found in the institutional reaction to arrest. Research shows that relations with school staff and teachers strongly influence student outcomes such as academic engagement, achievement, discipline, and dropout (Cernkovich and Giordano 1992; Jordan, Lara, and McPartland 1996). To the extent that the arrest of a student signals to teachers the difference between “normal” delinquency and serious misconduct, it may trigger adverse reactions by school staff and further alienation from school, in turn leading to high school dropout through the weakening of social bonds (see Hirschfield 2003).

In addition to such indirect pathways to dropout, criminal students may be directly excluded from school in the name of institutional accountability and school safety. Urban schools face an enormous challenge in fostering a safe learning environment while at the same time trying to provide an education to those students most at risk of crime and educational failure (Kirk and Sampson 2011). It is increasingly the case that schools, as rational organizations, promote policies and practices designed to demonstrate legitimacy and effectiveness (Hirschfield 2008b; Mayer 2005; Riehl 1999). Indeed, Mayer (2005) reports, based on interviews with expert informants in the Chicago Public Schools (CPS), that the primary reason why principals work to exclude criminally involved students from school is accountability: test scores, truancy rates, and graduation statistics are all believed to be adversely affected by re-enrolling students who have had contact with the criminal justice system. Moreover, one component of the “No Child Left Behind” Act is a mechanism providing students in “persistently dangerous” primary and secondary schools the opportunity to attend a safe school. One means to demonstrate legitimacy is to exclude those students who detract from the school’s appearance as a safe, effective school.

For a school to act on knowledge of a student's criminal record necessarily requires that the principal or school staff be aware of the arrest. Even though juvenile records are typically sealed, there are a variety of routes by which school officials may become aware that a student was arrested. First and foremost, one-quarter of arrests of Chicago juveniles occur on school grounds (Chicago Police Department 2006). The highly visible nature of on-campus arrests leads to much stigmatizing potential. Recent ethnographic evidence also reveals an increasingly visible role for police discipline within the school (Nolan 2011). Second, Chicago youth who are detained in a juvenile facility must attend an alternative high school operated by CPS while detained. Upon that individual's release from detention, the alternative high school contacts the school the student attended prior to arrest to notify the school of the student's location (Mayer 2005). Third, a sizable number of delinquency cases that are referred to the juvenile court following arrest will result in a sanction of probation, with a condition mandating that the youth attend school. Schools may thus become aware of a student's criminal behavior if the probation officer checks on the student's attendance. Lastly, although direct evidence is hard to come by, it is likely that teachers become aware of student gossip and social interactions about who gets in trouble with the law.

In Chicago, once school officials are aware of a student's arrest, there are exclusionary policies in place that can be used to expel the student. For example, students in violation of Group 5 or Group 6 acts of misconduct under the CPS Student Code of Conduct may be expelled from school and assigned to Alternative Safe Schools (CPS 2009b). Group 5 and Group 6 acts involve serious criminal behavior either on or off school grounds, which may include arrest. As Mayer (2005:4) observed through interviews with school officials, such exclusionary practices are readily used: "Informants inside and outside of the Chicago Public Schools opined that court-

involved youth were unwelcome at regular public schools and that the schools found ways to exclude them.” Even though juvenile arrest has become relatively commonplace in some schools and neighborhoods, school officials still have ample reasons—including accountability and school safety—for excluding problem students.

Hirschfield (2003; 2008a) offers a potentially contrasting view about the use of exclusionary practices: arrests are so common among the students of some schools that arrest has become normalized, with little stigmatizing influence. He presents evidence from qualitative interviews that show that arrested students reported little resistance from schools to returning or re-enrollment following their criminal sanctioning. Hirschfield (2003:347) notes that many arrests of Chicago youths are for petty crimes and even “bogus” charges, and concludes that arrests “become an unreliable gauge of student character.” We agree, but even if an arrest merely validates a pre-existing “deviant” reputation and does not further affect the student’s reputation, this official validation may be the ammunition necessary for a school to initiate exclusionary practices against students they have long assumed were deviant. Hirschfield’s (2003; 2008a) student-focused account is thus not as divergent as it might first appear from Mayer’s (2005) evidence garnered from school officials about the use of exclusionary practices. In particular, informal status hierarchies among students might not penalize arrest, but to those charged with institutional control, arrest constitutes an official marker of state judgment (see also Nolan 2011). To the extent that reactive exclusionary policies of expulsion or assignment to alternative programs creates educational instability, frays a student’s bonds to school, or facilitates association with deviant role models, school dropout may be the end result (see, e.g., Bowditch 1993; Kelly 1993; Skiba and Peterson 1999).

In addition to the factors such as school exclusion practices that push arrested teens to drop out, time spent moving through criminal case processing (i.e., arrest, detention, prosecution, and probation) is time lost from the educational process (Hirschfield 2003; 2009; Sullivan 1989). Even if students are allowed to remain in school following arrest, they may miss so many classes and exams because of criminal case processing that they inevitably fail a grade. Given that grade retention is one of the most robust predictors of school dropout (see, e.g., Janosz et al. 1997; Rumberger 1987), the end result of time away from the classroom could be dropout. In addition to dropping out owing to grade retention, students may be automatically dropped from school because of excessive absences from, for example, time spent in a juvenile detention facility (Allensworth and Easton 2001; CPS 2006).

Finally, in addition to the largely involuntary paths to dropout such as school exclusionary policies, a student may voluntarily drop out of high school following arrest because of a loosening of his or her social bonds to school and school actors. Moreover, a student may voluntarily dropout because he or she rationalizes that the benefits of education are diminished given the stigma of a criminal record. As Morgan (2005) argues in his revision of the Wisconsin model of status attainment (Sewell, Haller, and Portes 1969), educational attainment is the product of educational expectations and is therefore contingent on the factors that shape one's beliefs about the future. We argue that a criminal record is one such contingent factor. There is convincing evidence that contact with the criminal justice system, even when arrest results in acquittal instead of a criminal conviction, limits future employment opportunities (e.g., Pager 2003; Schwartz and Skolnick 1962). Therefore it is rational to presume that the returns to education would be diminished by a criminal record. If assessments about returns to education are a basis of educational expectations and educational expectations determine educational

attainment, as the Wisconsin model predicts, then arrest may inhibit educational attainment by lessening educational expectations.

College Enrollment

Although rarely studied, the negative educational consequences of arrest may extend beyond high school. For those individuals who graduate from high school despite an arrest record, their educational transcripts may be marred by poor attendance and grades because of time spent navigating the criminal justice process or because of disciplinary infractions, thereby limiting their competitiveness in the college admission and financial aid process. In this case, the relatively more open admissions standards at two-year colleges may present a more viable route to higher education than a four-year school. In addition, high school staff, particularly guidance counselors, may have little motivation for dedicating institutional resources toward preparing criminally inclined students for college. Similarly, if criminal arrest and subsequent sanctioning alter one's family and peer network, then loss of social support may render college attainment a relatively more elusive goal.

In addition to the many student-centered reasons underlying the potential negative consequences of arrest for college enrollment, institutional mechanisms may also contribute to a lower likelihood of enrollment. *The Chronicle of Higher Education* (Lipka 2010) reports that more than 60 percent of U.S. colleges consider applicants' criminal histories when making admissions decisions. The Common Application, which more than 450 U.S. universities and colleges use, added an admissions question in 2006 asking applicants if they had ever been convicted of a misdemeanor, felony, or other crime (Jaschik 2007; Common Application 2011). This information can then be used as a screening tool to deny admissions, presumably to

dangerous individuals.¹ Some individual schools, such as the University of Illinois, ask for information about pending charges as well. Other schools go further by asking some applicants to furnish criminal-background checks, which may include information about arrests as well as convictions. In 2009, the University of North Carolina-Wilmington asked 10 percent of its applicants to submit such a background check, and denied admission to all applicants who failed to submit the information (Lipka 2010). Regardless of whether educational institutions actually use criminal history information in admissions decisions, prospective applicants may assume that they do and therefore not even apply.

Policies denying educational benefits to ex-offenders are another form of institutional barriers to college enrollment. The Higher Education Act of 1965, as amended by the Higher Education Act of 1998, suspended higher education benefits for adults convicted of misdemeanor or felony drug charges (sale or possession of drugs). Denied benefits included student loans, Pell Grants, Supplemental Educational Opportunity Grants, and Federal Work-Study (GAO 2005).² Without financial aid, some proportion of prospective students will not enroll in college and some admitted students will not finish. Because racial-ethnic minorities are substantially more likely to be convicted of a drug offense than whites, these financial aid restrictions exacerbate educational inequality (Wheelock and Uggen 2008). In sum, there are

¹ The Common Application does not strictly require applicants to respond to questions about their criminal conviction if the conviction has been sealed, expunged, or otherwise ordered to be kept confidential, as is true with most juvenile records. Nevertheless, the application also requires applicants to answer questions about their disciplinary history in high school (e.g., probation, suspension, or expulsion). If an applicant was disciplined in high school because of an arrest, then information about the disciplinary violation and the relevant circumstances surrounding it must be included. The Common Application's focus on criminal conviction is a different level of sanction than arrest, but nonetheless illustrates that many institutions of higher education have at their disposal information on the criminal sanctions of potential admits, which can then be used as an evaluation criteria.

² This law was subsequently amended in 2006, easing the restrictions on students with past convictions while still denying education benefits to individuals who were receiving aid at the time of their drug conviction.

several institutional mechanisms in place that make arrest, and especially a subsequent conviction, a significant hurdle for college attainment.

PRIOR EVIDENCE

Although limited, initial answers exist to some of the questions posed in the introductory section. Bernburg and Krohn (2003) find that police intervention, in the form of arrest and contact with the police, decreases the odds of high school graduation by over 70 percent. Sweeten (2006) finds that a first arrest in high school nearly doubles the likelihood of dropping out. Hjalmarsson (2008) estimates that arrested individuals are 11 percentage points less likely to graduate high school than those non-arrested. De Li (1999) and Tanner, Davies, and O’Grady (1999) similarly find a negative relationship between criminal justice contact and educational attainment.

Although these studies are informative, important questions and challenges remain. First, the observed correlations between arrest and school dropout or graduation may be explained by alternative, unmeasured factors. For example, low self-control, a lack of parental supervision, deviant peers, or neighborhood disadvantage may inflate the estimated relation between arrest and educational attainment in these studies. There are surprisingly few studies that account for such confounding, especially in a life course or longitudinal framework. Hjalmarsson (2008) shows that this may be consequential, estimating in a sensitivity analysis that the observed relation between arrest and high school graduation in her study would all but disappear because of unobserved factors that influence both graduation and arrest. Absent an experiment, which would pose its own challenges to causal inference and policy relevance (Manski 2009), the challenge in disentangling the relation between arrest and educational attainment is to assemble a

data repository that contains information on the many individual, family, peer, neighborhood, and school factors that jointly predict juvenile arrest and educational attainment.

A second challenge is the lack of empirical work that examines the effect of arrest on college enrollment.³ What little information that is available is largely descriptive and provides estimates of the number of prospective students affected by “tough on crime” policies designed to deny financial aid to drug offenders (GAO 2005; Wheelock and Uggen 2008). Given the importance of a college education for future employment and earnings, it is imperative to understand to what extent arrest influences this aspect of the transition to adulthood.

A recent study by Hirschfield (2009) addresses many of the limitations of prior research on educational attainment, at least with respect to high school dropout, and provides an important advance in understanding the consequences of arrest. Using a variety of quasi-experimental analyses designed to reduce the potential for selection bias, Hirschfield finds considerable evidence that arrest during high school leads to school dropout. Hirschfield’s sample is taken from an evaluation of Comer’s School Development Program in Chicago (see Cook, Murphy, and Hunt 2000), which targeted public schools in severely segregated and impoverished neighborhoods of Chicago. What remains to be tested about the consequences of juvenile arrest is the effect—net of confounding influences—in a representative sample of adolescents, neighborhoods, and public schools that characterize contemporary urban school districts in the United States, and whether and why any collateral damage from an arrest influences both secondary and post-secondary educational attainments.

³ Tanner et al. (1999) investigate the association between “contact with the criminal justice system” and college graduation using NLSY79 data. However, their measure of “contact” combines unsanctioned contact (stopped by police) with sanctioned contact (booked, charged, and/or convicted of a crime), where unsanctioned contact makes up the bulk of the total (Bureau of Labor Statistics 2008). As our theoretical discussion demonstrates, there are numerous reasons to expect that an official criminal record (and not merely being stopped by the police) stigmatizes youth and could lead to educational disruption. For theoretical reasons, we focus on the specific effect of an arrest record on educational attainment.

Strategy and Summary of Hypotheses

An understanding of inequalities in the transition to adulthood and beyond necessitates situating our study within several literatures: life course, stratification, education, and criminology.

Important work has disentangled the stratifying consequences of incarceration (e.g., Western 2006), yet, in our view, a focus on incarceration tends to overlook important events taking place earlier in the life course that put some individuals on the path to prison. Indeed, the age of onset of criminal offending for prisoners is in the teens (Blumstein et al. 1986), so to better understand the origins of life-course disadvantage, we start there. Our strategy is thus to shift the focus to late adolescence and assess whether and why early contact with the criminal justice system delays or alters the transition to adulthood for American youth in high school completion and college enrollment. We test the following hypotheses:

Hypothesis 1: Arrest has an independent, positive effect on high school dropout above and beyond the influence of individual, family, peer, neighborhood, and school correlates.

Hypothesis 2: The effect of arrest on high school dropout is mediated by declines in educational expectations, school attachments, and friend support following arrest.

Hypothesis 3: Arrest has an independent negative effect on enrollment in college, net of the effect on high school graduation.

DATA

We use a multi-wave research design that combines individual-level data from the Project on Human Development in Chicago Neighborhoods Longitudinal Cohort Study (PHDCN-LCS), the Chicago Police Department, the Illinois State Police, and the Chicago Public Schools (CPS) with

neighborhood- and school-level data from the U.S. Census, CPS, and the PHDCN Community Survey. This unique assemblage of data brings together information on the organization and functioning of neighborhoods, families, and schools with data on individual-level characteristics and behaviors.

As part of the PHDCN-LCS, seven cohorts of children and their primary caregivers were randomly selected based on a clustered multi-stage probability design and interviewed up to three times.⁴ Wave 1 of the survey was completed between 1995 and 1997; wave 2 between 1997 and 2000; and wave 3 between 2000 and 2002. The interval between interviews was about 2.5 years. The focus of our analysis of high school dropout is on the 12-year-old and 15-year-old cohorts; these youths were approximately 18 and 21 years old by the end of the data collection in 2002. For our examination of college enrollment, we draw on data from the 15-year-old and 18-year-old cohorts, who were approximately 21 and 24 years old by the end of the data collection. The PHDCN-LCS data contain a wealth of information on youth and family characteristics, including data on IQ, school enrollment, college enrollment, grade retention, disobedience in school, family structure and supervisory processes, parental educational attainment and sociodemographic characteristics, peer characteristics, and criminal offending (see Tables 1 and 2 for a list of the youth, family, and peer characteristics we measure). Importantly, these cohort data also contain indicators of youths' neighborhood of residence and school of attendance, allowing us to combine the data with other information about the characteristics of Chicago neighborhoods and schools.

⁴ Dwelling units were selected systematically from a random start within enumerated city blocks. Within dwelling units, all households were listed, and age-eligible participants (at baseline the target was household members within six months of age 0, 3, 6, 9, 12, 15, or 18) were selected with certainty. The resulting sample is evenly split by gender and ethnically diverse, with 16 percent European American, 35 percent African American, and 43 percent Latino. When sampling weights are applied participants are representative of children and adolescents living in a wide range of Chicago neighborhoods in the mid-1990s (see also Kirk 2008).

From the PHDCN-LCS, we use an indicator of *college enrollment* as one of our dependent variables. This measure is taken from the third wave of the PHDCN-LCS and indicates whether the respondent was enrolled in a four-year college or graduate program (which presumably requires that the student had previously enrolled and graduated from college) at any point during the data collection. In a supplementary analysis, we broaden the measure to include enrollments in a two-year college as well.

We measure *educational expectations*, *school attachment*, and *friend support* from wave 3 PHDCN-LCS survey responses, and assess whether these measures mediate the association between arrest and school dropout. To measure educational expectations, respondents were asked “how far do you think you will actually go in school” with ordered response categories ranging from “8th grade or less” to “more than college.” For school attachment, respondents were asked whether they like school, whether grades are important, whether they get along with their teachers, whether doing homework is useful, and whether they usually finish their homework. For friend support, students were asked whether they have friends who they can relax around, who trust and respect them, who share similar views and enjoy similar activities, and who they can confide in.

Data on neighborhood-level social organization and social-interactional processes come from the 1995 PHDCN Community Survey, and neighborhood measures of racial-ethnic composition, concentrated poverty, affluence, residential stability, and immigrant concentration derive from the 1990 U.S. census. The Community Survey yielded a probability sample of 8,782 residents across the 343 neighborhood clusters in Chicago who responded to a series of questions about the characteristics of their neighborhood environments. Data on school sociodemographic characteristics, including enrollment,

poverty, mobility, English proficiency, and the racial-ethnic composition of the student body, come from the CPS Office of Research, Evaluation and Accountability.

With the use of key identifiers, such as social security number, name, and birth date, we linked CPS student enrollment records from 1990 to 2005 with the PHDCN-LCS data. One of our dependent variables, *school dropout*, is drawn from these CPS student records. These data indicate precisely when CPS registered the dropout, allowing us to determine temporal ordering with arrest events. From the CPS student records, we have determined under what circumstances a student exited the CPS system. These include if students completed high school, if they transferred to a non-CPS school, if they were “lost” by the CPS system (i.e., former students who could not be located by CPS), or whether they dropped out of CPS (without transferring to a different school district). To develop our dropout measure, we include students designated as dropouts by CPS as well as those students who obtained a GED and students who could not be located by CPS.⁵ We use the final student attendance record to determine dropout—e.g., if a student dropped out, re-enrolled, and then graduated, we treat that student as a graduate and not a dropout. Only if a student’s final disposition in the system meets our criteria of dropout do we measure it as dropout. Note that, by law, students cannot drop out of the CPS system prior to age 16, and CPS cannot drop them from school (e.g., for truancy) before this age.⁶ Because our dropout measure is derived from school records, as opposed to survey responses, we are able to track the educational progress of CPS students even if they did not participate beyond the first

⁵ Orfield and colleagues (2004; see also National Research Council and National Academy of Education 2011) highlight a number of individual-level issues with the computation of dropout statistics, which lead to widely varying estimates of dropout even from the same data sources. One issue is whether to consider “lost” students as dropouts or as students who relocated to another school district. For the purposes of this study, we adhere to practices established by CPS and the Consortium on Chicago School Research, and treat “lost” students as dropouts.

⁶ This policy changed effective January 1, 2005. Now students must be 17 to drop out. However, all analyses are based on observation years prior to the policy change, when age 16 was the cutoff for dropout eligibility.

wave of the PHDCN-LCS data collection.

Finally, we linked official arrest records from 1995 to 2001 from the Illinois State Police and the Chicago Police Department with the PHDCN-LCS data, and use these arrest records to construct our treatment variables for analyses of school dropout and college enrollment.⁷ The arrest data contain information on all juvenile and adult arrests of sample youth occurring throughout the state of Illinois during the specified time period. Our first treatment variable, for the analysis of high school dropout, is a binary variable indicating whether the student had been arrested at any point while enrolled in high school. Most of the arrestees in our data were arrested for the first time during high school, as opposed to earlier. With information on the date of arrest, we are able to determine if the arrest occurred prior to dropout. Our treatment group, which makes up 13 percent of the sample, consists of students arrested while enrolled in high school; our control group consists of students who were not arrested while enrolled in high school.⁸ Our second treatment variable, for the analysis of college enrollment, is a binary variable indicating whether an adolescent had been arrested at any point from age 15 to 18. Twelve percent of the sample was arrested at least once during this span.

Our analytic sample for the analysis of high school dropout consists of respondents from the 12- and 15-year-old PHDCN-LCS cohorts who were enrolled in the Chicago Public Schools during ninth grade, and who then either completed their schooling in CPS or dropped out of CPS ($N = 659$). We exclude students who transferred to another school outside of CPS. For the

⁷ Arrest records include only formal arrests, not informal “station adjustments.” A station adjustment is an informal handling of arrests for youths with a limited prior history of delinquency, where the adjustment most often results in a stern warning from the police and then the unconditional release of the youth without any prosecution or supervision (Hirschfield 2009; Kirk 2006).

⁸ If after graduating or dropping out, a former student was arrested for the first time, that student would still be part of the control group because he or she had not been arrested while enrolled. We do include in our control group six respondents who were arrested prior to high school but not arrested during high school. Nevertheless, we replicated our analyses while excluding these six individuals from the control group, with almost identical results.

analysis of college enrollment, our sample consists of respondents from the 15- and 18-year-old PHDCN-LCS cohorts who attended a CPS high school and either graduated or obtained a GED ($N = 355$).

We handle missing data through several steps. First, to account for any bias associated with attrition from wave 1 to wave 3 of the PHDCN-LCS, we use attrition weights in our analyses of college enrollment as well as the intervening mechanisms between arrest and dropout (i.e., educational expectations, school attachment, and friend support). We constructed weights by estimating the probability of responding at wave 3 as a function of the wave 1 covariates displayed in Tables 1–3. Because we are able to measure school dropout from CPS records, we can examine this outcome for the entire analytic sample irrespective of attrition in the PHDCN-LCS. Therefore, we do not use attrition weights in analyses of dropout. Second, for individuals who were interviewed but who did not provide information for a specific variable, we use the *ice* command in Stata to implement the multiple imputation by chained equation (MICE) algorithm to create five imputed data sets (see Royston 2004; van Buuren, Boshuizen, and Knook 1999). For our propensity score analyses, we follow Hill’s (2004:13) multiple imputation matching strategy and calculate a propensity score of arrest for each observation in each of the imputed data sets. We then average the propensity scores for each respondent across the five imputed data sets.

ANALYTIC MODELS

Our analyses follow three paths. First, we wish to determine what would happen to the educational trajectory of the same individual under two different circumstances, one in which the student was arrested and the other in which the student avoided arrest. Yet, we only observe one

of these potential outcomes (i.e., a student is either arrested or not). Given that we observe only one outcome, we employ propensity score matching to estimate the effect of arrest on school dropout and college enrollment. This approximates an experimental design in which treated youth (i.e., arrested) are equivalent to control group youth (i.e., not arrested) (Morgan and Winship 2007; Rosenbaum 2002). See Appendix A for a detailed description of our propensity-matched design.

Second, we use Rosenbaum's (2002; 2010) bounding approach to examine the sensitivity of our propensity-matched inferences to hidden biases (see also Becker and Caliendo 2007; DiPrete and Gangl 2004). This approach allows us to determine how strongly an unmeasured confounding variable must influence selection into treatment to undermine our inferences about the causal effect of arrest on dropout and college enrollment. See Appendix A for further description of this bounding methodology.

Third, if arrest is causally related to subsequent school dropout, there are several potential mechanisms why arrest is predictive of dropout. We explore three: declines in educational expectations, school attachment, and friend support.

RESULTS

Descriptively, our data reveal that among the CPS students who steered clear of the juvenile justice system, 64 percent went on to graduate high school.⁹ In contrast, a mere 26 percent of arrested students graduated high school. Of those young adults without a criminal record who graduated high school or obtained a GED, 35 percent enrolled in a four-year college. For

⁹ By comparison, CPS reports an overall five-year graduation rate in 2004—i.e., the number of graduates divided by the number students enrolled in ninth grade five years earlier—of 50.1 (CPS 2009a). Nationally, the graduation and dropout rates in Chicago are comparable to many other large urban districts (see National Center for Education Statistics 2008a: Table A-13; Orfield et al. 2004).

arrestees, 16 percent subsequently enrolled in a four-year college.¹⁰ These gaps, though unadjusted for differences between arrestees and non-arrestees on the other correlates of education, do suggest that arrest has severe consequences for the prospects of educational attainment. Given the large differences in both high school graduation rates and college enrollment, arrest is a snare that reverberates at numerous points on the transition to adulthood.

The considerable differences in graduation and college enrollment rates between arrestees and non-arrestees are not the only points of divergence. Table 1 compares individual and demographic characteristics of arrested and non-arrested PHDCN sample members, before and after matching on propensity score. Focusing on the “unadjusted” prematch differences, the comparison reveals that arrestees are more likely to be male than non-arrestees, and less likely to be Mexican or white. These demographic characteristics of our sample are similar to data reported by the Chicago Police Department (2006) on the demographic characteristics of all juvenile arrests citywide. Little difference exists between arrestees and non-arrestees in IQ and in student mobility. However, arrested youths are more likely to have failed a grade and to have been enrolled in remedial or special education. Thus, even prior to contact with the criminal justice system, eventual arrestees showed signs of educational difficulties. Our ensuing analyses seek to determine whether arrest further undermines educational attainment. Arrested youth also tend to have less self-control and persistence, and they are more commonly sensation seeking. In terms of problem behavior, those arrested tend to be more aggressive (this scale includes disobedience in school), yet the difference is not statistically significant. Not

¹⁰ Data from the National Center for Education Statistics (2008b: Table 204) reveal that in 2002, which is the last year of the PHDCN-LCS data collection, 32.9 percent of high school completers were enrolled in a four-year degree granting postsecondary institution. Overall, then, the data for Chicago are similar to national estimates.

surprisingly, arrested adolescents are significantly more likely to engage in violent offending, property crime, and drug distribution than those not arrested.

[Table 1 about here]

Table 2 displays summary statistics for family and peer covariates by group. Results show significant differences across groups in immigrant generational status as well as differences in the proportion of students with married parents. Surprisingly, results reveal little difference between groups in terms of family supervision and support. Arrested adolescents are more likely to have mothers with substance abuse problems and are more likely to have experienced parent-child conflict. In terms of peer influence, arrestees are significantly and substantially more likely to associate with deviant peers, a finding that is consistent with a long line of criminological research (see Burgess and Akers 1966; Sutherland 1947; Warr 2002).

[Table 2 about here]

Table 3 reveals significant differences in the racial-ethnic composition of respective neighborhoods and schools. Arrested youths tend to reside in neighborhoods characterized by substantially more poverty and violent crime and substantially less immigration. Arrested youths reside in neighborhoods with more neighborhood organizations (e.g., tenant associations, drug or alcohol treatment programs, or family health services) than do non-arrestees. As expected, collective efficacy is weaker in neighborhoods where arrested youths tend to reside.

[Table 3 about here]

Propensity-Matched Analysis of School Dropout

Analyses presented thus far reveal that arrested students are substantially more likely to drop out of school than non-arrested students. Analyses also indicate that arrested and non-arrested students, on average, differ on numerous individual, family, peer, neighborhood, and school characteristics. Many of these differences are also associated with school dropout. For instance, parental marital status, family structure, and socioeconomic status are strong predictors of numerous types of problem behavior, including dropout and arrest (see, e.g., Cairns, Cairns, and Neckerman 1989; Ekstrom et al. 1986; Kirk 2008; Rumberger 1983). Therefore, it is important to determine if any apparent relationship between dropout and arrest is simply because each outcome has a similar set of causal predictors.

We attempt to isolate the specific effect of arrest on dropout by matching and comparing arrested and non-arrested sample members who are otherwise similar to one another in their frequency of criminal offending and all of the pretreatment characteristics displayed in Tables 1, 2, and 3. It is important for our analysis that numerous factors outside the control or background of an individual influence whether a crime will culminate in an arrest. Two key determinants include whether the crime is known to the police and police discretion. Most crimes are not in fact known to the police, and the police arrest proportionally few known suspects of a crime (Black and Reiss 1970). Thus, unlike many other behaviors under the control of an individual (selection), the arrest decision, which we conceptualize analytically as the “treatment,” lies with the police and is based on a host of external and often idiosyncratic factors in addition to the criminal behavior and other characteristics of the individual. For example, during his time as a Baltimore police officer, Moskos (2008) observed substantial variation in the number of arrests made by the officers in his squad. Officers patrolled the same drug-infested areas of the Eastern District of Baltimore and worked under the same sergeant, yet some officers made up to a couple

dozen arrests per month while many others averaged none or one arrest per month. Moskos argues that whether someone is arrested following a crime is largely a function of the characteristics of the officer, not the suspect.

It is in this sense that juvenile arrest has a random component, making it entirely likely that two otherwise equivalent individuals in the PHDCN sample, in terms of criminal offending and other pre-treatment covariates, end up with different officially defined fates because one was unfortunate enough to get arrested following the commission of a crime while the other avoided arrest. Indeed, much attention in the criminological and juvenile justice literature has focused on the seemingly random and thus “inequitable” nature of juvenile arrest outcomes. As a result, there are strong substantive reasons to expect an overlap in the likelihood of arrest between the treatment (arrested) and control (not arrested) groups. Empirically we validate this assumption by examining the overlap in propensity scores across groups (see Appendix Figure 1). By matching with replacement within a caliper of 0.03 (caliper refers to a maximum tolerance of distances between propensity scores of the treated and control subjects), we are able to match 79 of the 85 arrested youth to at least one control observation.¹¹

Before proceeding to estimate the effect of arrest on school dropout, we first determine whether our matching procedure has produced balance across the treatment and control groups on observed covariates. Our objective is to ensure that the treated and control groups are similar, on average, across all observable covariates. The post-match *t*-statistics and corresponding *p*-

¹¹ In total, we used 115 control observations in the matching procedure for school dropout. Six arrestees in the sample had a propensity to be arrested for a crime that was not similar to any of the non-arrested youths (i.e., not within a caliper of 0.03), and therefore could not be statistically matched to any of the non-arrested youths. These six youths all had a predicted probability of being arrested of at least 0.70, and four youths had a probability greater than 0.90. Our analysis of school dropout excludes the six unmatched arrestees. Whereas our full sample ($N=659$) is representative of youths living in Chicago neighborhoods in the mid-1990s, our matching procedure necessarily subsets the data to those individuals who were arrested and their statistical matches. The 194 youths that comprise the set of treated (79) and control (115) cases used in our analysis are not necessarily representative of all Chicago youths.

values in Tables 1–3 reveal that among some 82 covariates used to estimate the propensity score, not one significant difference emerged between the treated and controls in our final matched sample. In addition, with few exceptions, matching on propensity score produced decreases in bias (see Appendix A for a discussion).¹²

With common support and balance, we proceed with our comparison of dropout across groups. Our propensity-based results reveal that the probability of dropping out of school is 0.22 greater for arrested adolescents relative to otherwise identical individuals who were not arrested. This difference, which is known as the average treatment effect on the treated (ATT), is statistically significant.¹³ On average, arrested youth have a 0.73 probability of subsequently dropping out of public school. In contrast, youths who avoid the snare of arrest have a probability of dropping out equal to 0.51. The data thus reveal that the likelihood of completing high school is tragically low overall for students in the CPS system. Yet for those youths who commit crimes and get caught, the repercussions of criminal justice sanctioning drastically limit the already dismal chances for high school graduation.

Sensitivity Analyses, Effect of Arrest

To test the sensitivity of our causal estimates to the specification of our matching procedure, we reran the analyses after matching treated and control youths without replacement, using one-to-one matching. With this revised procedure, we still find a substantial, albeit slightly more conservative, difference in the probability of dropping out of high school between arrested youths and similar control youths. Arrested adolescents have a 0.74 probability of subsequently

¹² The exceptions, mainly among select family variables, occurred because mean values across treated and control groups were nearly identical prior to matching (e.g., paternal criminal record), and matching yields a slight increase in the difference. Yet increased differences across groups after matching are not statistically significant.

¹³ The ATT provides an estimate of the effect of an arrest on those individuals arrested, as opposed to a randomly selected youth from the population.

dropping out of public school, whereas control youths have a probability of dropping out equal to 0.55. In sum, we find a substantial effect of arrest on high school dropout, and this finding holds under alternative specifications of our matching procedure.

Despite our efforts, it is still possible that there are unobserved confounders that would change the results if included. Therefore, we estimate a sensitivity analysis based on Rosenbaum's (2002) bounding strategy to address just how substantial unmeasured confounding influences would have to be present to substantially alter our inferences about the effect of arrest on dropout. In this procedure, we use one-to-one matching without replacement—i.e., the matching specification from our more conservative results—to implement the sensitivity analysis.

As described in the methodological discussion in the Appendix, Γ in Table 4 refers to the factor increase in the odds of treatment (arrest) due to unobservable factors beyond the influence of the estimated propensity score. At $\Gamma = 1$, we assume there are no hidden biases, and therefore conclude that arrest has a significant positive effect on school dropout ($Q^+ = 2.400, p = .008$). Positive selection bias would occur if those students most likely to get arrested tend to have higher dropout rates even in the absence of arrest. At $\Gamma = 1.2$, we are examining the effect of hidden bias which would increase the odds of arrest for an arrested individual by an additional 20 percent relative to an untreated individual, after accounting for the propensity score. Even under this scenario, we still find a significant positive effect of arrest on dropout ($Q^+ = 1.842, p < .033$). It is not until a Γ above 1.25 that unobserved heterogeneity is severe enough to render the treatment effect of arrest no longer significant at $p < .05$. As a comparison, we find that violent offending and residence in a neighborhood of concentrated poverty increase the odds of arrest by an additional 20 to 30 percent, after controlling for a propensity score that excludes these factors.

We believe it is unlikely that there is an unobserved factor beyond the 82 we already include in our propensity score estimation *and* that would be just as influential as factors like violent offending and concentrated poverty in the estimation of arrest, but that is what it would take (i.e., a $\Gamma > 1.25$) for our observed treatment effect to disappear.

[Table 4 about here]

Mechanisms Underlying the Effect of Arrest

There are several potential mechanisms that may explain why arrest leads to school dropout. In Tables 5–7 we explore three potential mediating influences. First, the stigma of a criminal record could lessen one’s expected returns to education, which then influences educational expectations and ultimately educational attainment (Morgan 2005). Second, arrest may weaken a student’s attachment to school, which then increases the likelihood of dropout. Third, a criminal arrest may adversely affect social relationships, leading to rejection by prosocial peers (Lemert 1951; see also Bernburg, Krohn, and Rivera 2006). In exploring this third mechanism, we draw on the child development literature—which has shown that children with conduct disorders are more likely to be rejected by their peers—as a basis for arguing that an arrest has implications for a youth’s peer opportunities (Dodge 1983; Dodge and Pettit 2003). Beyond the effect of a stigma on peer relationships, an arrest that leads to juvenile detention or school transfers may adversely affect peer relationships by pulling an individual away from his or her social network. A result is that a lack of attachment to and support from friends may lead to school dropout.

To test the mediating influence of the three mechanisms, we first regress each (educational expectations, school attachment, and friend support) on arrest, controlling for the propensity of arrest. Second, we use logit regression models to estimate the effect of each

mechanism on school dropout, controlling for arrest and the propensity of arrest (see Morgan and Winship 2007: Chapter 8).¹⁴ We do not consider educational expectations, school attachment, or friend support to be isolated mechanisms for the causal effect of arrest on dropout because each is determined by other factors besides criminal arrest that may also be related to our outcome variable. Yet by conditioning on the propensity score, we seek to block observed variables that confound our ability to estimate the effect of arrest on each of the three mechanisms as well as the effect of each mechanism on dropout.

Table 5 presents the results of our analysis of educational expectations as a causal mechanism. Model 1 reveals a nonsignificant association between educational expectations and arrest, which suggests that educational expectations will have little mediating effect on school dropout. Turning to estimates of school dropout, consistent with our propensity-matched results we see in Model 2 that an arrest record is significantly and substantially predictive of later school dropout. Model 3 adds the measure of educational expectations to determine whether this measure mediates the association between arrest and school dropout. Results reveal that educational expectations are negatively predictive of dropout; the greater the expectations, the less likely that dropout results. Given that arrest is unrelated to educational expectations (Model 1), it is unsurprising that expectations do not mediate much of the effect of arrest on dropout. The coefficient for arrest declines by just 2 percent (from 2.092 to 2.045) from Model 2 to 3 once adding educational expectations to the equation.

¹⁴ This analysis does not match arrested and nonarrested youth by propensity score; rather, it uses the propensity score as a control variable in an analysis using the full sample. Analyses of educational expectations and school attachment are limited to the 12-year-old cohort ($N = 335$). Data on these two mechanisms are not available for the 15-year-old cohort. Analyses using friend support are based on both the 12- and 15-year-old cohorts ($N = 659$). Thus, we have relatively more statistical power to detect the relationship between arrest and friend support, and the mediating effect of friend support on school dropout.

[Table 5 about here]

In Table 6, we examine whether arrest leads to school dropout by weakening a student's attachment to school. Similar to results for educational expectations, Model 1 reveals a nonsignificant association between school attachment and arrest. This finding suggests that arrest does little to undermine a student's attachment to school. Model 2 provides the baseline results and Model 3 adds the measure of school attachment. Results reveal that school attachment is negatively predictive of dropout, but does little to mediate the effect of arrest. The coefficient for arrest declines by just 4 percent relative to Model 2.

[Table 6 about here]

Table 7 presents findings on friend support. Because of an individual's arrest record, friends may come to reject the arrestee, thereby leading to a weakening of supportive relationships. Model 1 provides partial support for this assertion. We find a marginally significant negative relationship between arrest and friend support. Models 2 and 3 demonstrate the same general theme depicted in Tables 5 and 6; friend support is significantly and substantially predictive of school dropout, but does little to mediate the effect of arrest on dropout. The coefficient for arrest declines just 4 percent between Models 2 and 3.

In sum, educational expectations, school attachment, and friend support have limited roles in explaining the effect of arrest on later school dropout. Thus, sorting out the potential mechanisms underlying the observed effect of arrest remains an important area of investigation. Perhaps it is telling that each of the three mechanisms we investigated likely contributes to a student's voluntary decision to drop out. One way to interpret their general lack of relevance as mediators is that arrest leads to dropout not because of voluntary mechanisms, but because arrested students are involuntarily pushed out of school through enforcement mechanisms. In

this sense, those students whose reputations are most stigmatized by their involvement in the criminal justice system may be the ones most likely to drop out of high school.

[Table 7 about here]

The Effect of Juvenile Arrest on College Enrollment

Our findings thus far reveal that arrest is a negative turning point that can derail a youth's prospects of graduating high school. Theoretically, we expect that the consequences of arrest during adolescence stretch beyond secondary schooling, by lessening the likelihood of further educational attainments even for those students who do manage to complete high school. We are unaware of any research which has systematically investigated the effect of arrest on college attainment. Thus, we seek to isolate the specific effect of arrest on college enrollment by matching arrested and non-arrested sample members who graduated high school or obtained a GED who are otherwise similar to each other with respect to pretreatment characteristics. To achieve balance across the treatment and control groups on observed covariates, we match each arrestee with up to two control youths and use a caliper of 0.03 to ensure that the matches for each treated subject are suitable. With these matching specifications, we are able to match 38 out of 43 arrested youths to at least one control youth.¹⁵ As with our matching for the analysis of school dropout, post-match *t*-statistics reveal no significant differences between the treated and control groups on pre-treatment covariates.¹⁶

Our propensity-based results for both high school dropout and college enrollment are compared in Figure 1. The probability of enrolling in a four-year college is 0.16 lower for

¹⁵ In total, we used 59 control observations in the matching procedure for college enrollment.

¹⁶ Recall that our analytic sample for the analysis of college enrollment includes members of the 15- and 18-year-old cohorts while our analysis of high school dropout was based on the 12- and 15-year-old cohorts. Therefore, we conduct a new matching procedure on this different analytic sample. In this case, matching is based on a slightly varied mix of pre-treatment characteristics because the 18-year-old cohort was administered a slightly different set of survey questions than the 12- and 15-year-old cohorts.

arrestees relative to otherwise identical individuals who were not arrested, a gap just smaller than the difference in high school dropout. On average, youths with an arrest record are not only much more likely to drop out, at over 70 percent, but they have only a 0.18 probability of enrolling in a four-year college. In comparison, non-arrestees have a probability of college enrollment equal to 0.34.¹⁷

We also broadened the analysis to include two-year colleges in the dependent variable and find no significant difference between arrestees and non-arrestees in college enrollment using this more inclusive measure. Thus, an arrest record, independent of its effect on high school attainment, does not adversely affect enrollment in two-year colleges, but it does limit one's opportunity to pursue a degree in a four-year institution. Thus, one consequence of arrest is that it seems to narrow the postsecondary schooling opportunities available to individuals to those institutions with relatively open admissions standards such as community colleges. To the extent that community colleges serve to stratify higher education (Brint and Karabel 1989), an arrest record may ultimately limit the labor market prospects of those arrestees who do manage to enroll in college, by curtailing their options for pursuing a higher education.

[Figure 1 about here]

We again test the sensitivity of our causal estimates to hidden biases. Given the direction of our results—a negative relationship between arrest and college enrollment—positive selection bias would cause our findings to be conservative. Negative selection bias would occur if those students most likely to get arrested tend to have lower college enrollment rates even in the absence of arrest. Thus, we focus on negative selection and the Q_{MH}^- statistic in Table 8. We

¹⁷ As with our analysis of school dropout, we tested the sensitivity of our causal estimates to the specification of our matching algorithm. When using one-to-one matching without replacement, we find that the probability of enrolling in a four-year college is 0.21 lower for arrestees relative to matched controls. Thus, the substantial gap in college enrollment appears robust to different specifications of our matching procedure.

find that the significant negative relationship between arrest and college enrollment may be biased if an unobserved variable increases the odds of arrest by an additional 20 percent for arrestees relative to non-arrestees, after accounting for the propensity score. Again, we emphasize that our model includes nearly 80 different individual, family, peer, neighborhood, and school predictors of arrest, including measures of criminal offending. It is hard to conceive of additional measures that would increase the odds of arrest by another 20 percent.

[Table 8 about here]

CONCLUSION

To understand the transition to young adulthood in the United States, it is increasingly necessary to grapple with the consequences of contact with the criminal justice system. Meaningful life events such as school completion, labor force entry, and family formation are sequentially related and interdependent, and educational experiences can have profound effects on the shape of the life course. Western (2006:92), for example, reports that among black male high school dropouts aged 22–30 in 2000, approximately 65 percent were jobless. Half of the jobless were incarcerated. The numbers are not much better for black males who graduated high school but did not enroll in college: 42 percent were jobless and more than 40 percent of this jobless group was in jail or prison. Certainly for black male dropouts, but even for black high school graduates (without any further education), joblessness and imprisonment are now normative in the life course. The alarming differences in employment, wages, and family life between the never incarcerated and the incarcerated and formerly incarcerated can thus ultimately be traced to educational disadvantage.

Our analysis shows that arrest in adolescence hinders the transition to adulthood by undermining pathways to educational attainment. Among Chicago adolescents otherwise

equivalent on pre-arrest characteristics, 73 percent of those arrested later dropped out of high school compared with 51 percent of those not arrested, a substantial difference of 22 percent. It seems unlikely that data limitations alone can explain the large gap. Our sensitivity analyses revealed that it would take an unmeasured confounding factor as consequential as violent offending or neighborhood poverty to overturn the difference, which we systematically investigated with one of the most comprehensive longitudinal studies to date. The educational repercussions of early exposure to the criminal justice system do not stop at high school. Among otherwise equivalent young adults with a high school diploma or GED, 18 percent of arrestees later enrolled in a four-year college relative to 34 percent of non-arrestees.

What explains the apparently large effect of arrest on the educational life of adolescents? This is a crucial question that yielded an uncertain answer. We found little evidence of declines in educational expectations, school attachment, or friend support as mediating mechanisms. Rather than construing these results as “non-findings,” however, we consider our analysis of theoretically plausible mechanisms to be a necessary analytic step toward disentangling why arrest is so consequential to educational attainment. Indeed, by ruling out the importance of such person-level mechanisms, we direct attention to the importance of institutional responses and the increasingly punitive “zero tolerance” educational climate (Nolan 2011) along the path to dropout. Institutional reactions to an arrest record may also work to narrow options available to college-seeking students, making community college the only viable option for higher education.

We recognize that our study was restricted to a single city, the public school setting, and a particular time period. Within the limitations of our research design, we nonetheless believe the main results, considered in light of the sensitivity bounding, support the inference that arrest

has substantive import for the educational attainment of students who attended the Chicago Public Schools. Although Chicago is of course only one case, it is the third largest U.S. city and broadly representative of the racial and ethnic diversity of urban school districts nationwide. Although a national sample would be desirable, we are not aware of any study with the density of information on individual adolescents and their multiple contexts in the transition to young adulthood—including the police, schools, *and* neighborhoods—that would furnish the kind of detailed empirical assessment we offer here.

We therefore suggest that more research is needed to examine why arrest appears to be so consequential to educational attainment. A focus on institutional responses to student criminality appears a particularly important and understudied avenue for future research. These responses would include both formal actions, such as expulsion for an arrest or denial of admission to college, as well informal responses, such as increased punitiveness on the part of teachers if the arrested student is subsequently disruptive in class. In addition to institutional responses, student absences, school and program transfers, and any resulting frustration with falling behind all deserve greater scrutiny. In line with this reasoning, we find that among the 22 (out of 85 total) arrestees in our sample who managed to graduate from a CPS high school, not one was incarcerated in a juvenile facility. Conversely, an arrest that results in a period of confinement in a juvenile detention facility virtually guarantees that a student will not finish high school. Every youth in our sample who spent time in a juvenile detention facility ultimately dropped out of high school. Although data limitations prevent us from examining the specific reasons, we suggest that time in juvenile detention makes stigmatization more likely and makes it difficult for a student to re-engage in the schooling process.

Another next step would be to assess the extent to which race, ethnic, and class differences in arrest account for group differences in educational attainment. In a similar vein, while juvenile arrest hinders educational advancement, its effect may not be uniform across social groups. “Second chances” may be unevenly distributed. Individuals in disadvantaged structural positions, because of race, poverty, and a lack of pro-social bonds, may be less able to avoid the snares of arrest (Sampson and Laub 1997).

With high school and even college graduation virtually a necessity for a successful transition to adulthood, we conclude that the evidence comes down on the side of viewing juvenile arrest as a life-course trap in the educational pathways of a considerable number of adolescents in contemporary American cities. That this snare appears to work independently of a number of traditionally hypothesized mechanisms raises troubling questions about the interaction of the criminal justice and educational systems.

APPENDIX A

Propensity Score Matching

We use propensity score matching to estimate the effect of arrest on school dropout and college enrollment. Imbalance contributes to a lack of comparability and equivalence between treatment and control groups—in the case here, between arrestees and non-arrestees. Imbalance between the groups occurs if there are differences in the pre-treatment characteristics of each group. Imbalance becomes a problem if there are differences across groups in confounding factors—i.e., characteristics of youths that are related to both the likelihood of arrest and educational attainment. If groups are imbalanced, then a comparison of the prevalence of school dropout and college enrollment across groups will not yield a valid estimate of the effect of arrest on educational attainments—some other difference between the groups *besides arrest* may account for outcome differences.

To resolve any issues of imbalance, we statistically adjust for differences between groups through propensity score matching (Morgan and Harding 2006; Morgan and Winship 2007). The propensity score is defined as the probability that a given youth receives the treatment (i.e., was arrested) given all that we observe about him or her and his or her family, peers, neighborhood, and school. It is a summary measure of the characteristics that could confound our ability to estimate the effect of arrest on dropout and college enrollment.¹⁸ We estimate the propensity of arrest for each student using a logit model with arrest as the binary

¹⁸ By conditioning on the propensity score, we seek to block back-door paths from our treatment condition, arrest, to our outcomes. Following Pearl (2000), Morgan and Winship (2007:69) define a back-door path as “a path between any causally ordered sequence of two variables that includes a directed edge...that points to the first [treatment] variable.” A back-door path may contribute to the association between the treatment and outcome variable, so blocking back-door paths is necessary to consistently estimate the effect of a treatment on an outcome.

outcome variable.¹⁹ We use 82 different covariates measured at the first wave of the data collection (displayed in Tables 1, 2, and 3) as predictors of arrest, including measures of the frequency of criminal offending (disaggregated by violent, property, and drug offenses) and relevant predictors of educational attainment (e.g., parental educational attainment and grade retention). We then calculate the predicted probability of arrest based on these covariates. By accounting for such an extensive set of confounders, we seek to eliminate the potential for hidden biases in our estimation of the treatment effect of arrest.

After estimating the propensity score, we match each treated subject (i.e., arrested) with up to three control subjects (i.e., non-arrested) with very similar propensity scores, to produce treatment and control groups that are indistinguishable except for the receipt of treatment once conditioning on propensity scores. In this procedure, we use matching with replacement—that is, each control subject can be matched to more than one treated subject. Matching with replacement generally increases the quality of matches (i.e., reduces bias), but also increases the variance of the estimate because fewer unique control observations are used to construct counterfactuals (Morgan and Winship 2007; Smith and Todd 2005).²⁰ Matched observations will not necessarily be similar on every single covariate, but they will be similar, on average, across all the covariates used to estimate the propensity of arrest. All methods must make assumptions, and propensity modeling is no exception. We assume that selection into treatment and control groups is strongly ignorable (i.e., assignment to control and treatment groups is random) after conditioning on the propensity to be arrested.

¹⁹ Because we use two different arrest measures in our estimation of dropout and college enrollment respectively, we necessarily estimate two different propensity scores to correspond to these arrest outcomes.

²⁰ Given this tradeoff between bias and variance, after undertaking our main analysis using matching with replacement, we conduct a sensitivity analysis that uses one-to-one matching without replacement.

After matching treated and control cases, we determine whether our matching procedure produces balance across the groups on observed covariates. This can be done by assessing the percent reduction in absolute bias and the mean differences across groups for each covariate after adjusting for propensity scores. Bias represents the mean differences across groups as a percentage of the square root of the average of the sample variances: $100 * (\bar{x}_T - \bar{x}_C) / (s_T^2 + s_C^2)^{1/2}$, where \bar{x}_T and \bar{x}_C are the sample means in the treated group and the control group respectively, and s_T^2 and s_C^2 are the respective sample variances (Rosenbaum and Rubin 1985).

Bounds for the Treatment Effect of Arrest

We use Rosenbaum's (2002) bounding approach to examine the sensitivity of our propensity-matched results to hidden biases (see also Becker and Caliendo 2007; DiPrete and Gangl 2004). If there is some level of hidden bias, then two individuals with the same observed characteristics will have differing likelihoods of receiving treatment (i.e., arrested) because of unobserved factors. Here we outline our approach to examining the sensitivity of results to such hidden biases.

The odds that an individual will receive treatment (arrest) is given by the following:

$$\frac{\Pr(Arrest = 1)}{1 - \Pr(Arrest = 1)} = \exp(\alpha + \beta X + \gamma U),$$

where X represents observed variables and U represents one or more unobserved variables. In this case, the variable U increases the probability of arrest by a factor equal to γ . For a pair of individuals, i and j , matched on propensity score (i.e., the same observed covariates X), where i ultimately is arrested and j is not, the ratio of odds of receiving treatment is given by:

$$\frac{\frac{P_i}{1-P_i}}{\frac{P_j}{1-P_j}} = \frac{\exp(\alpha + \beta X_i + \gamma U_i)}{\exp(\alpha + \beta X_j + \gamma U_j)}$$

Because i and j have the same set of observed covariates, X cancels out:

$$\frac{\exp(\gamma U_i)}{\exp(\gamma U_j)} = \exp[\gamma(U_i - U_j)]$$

If there are no differences in unobserved variables ($U_i = U_j$ for all matched pairs) or if unobserved variables have no influence on the probability of treatment ($\gamma=0$), then there is no hidden bias. Because we lack direct information on unobservables, we use a sensitivity analysis to evaluate whether our statistical inferences pertaining to the effect of arrest on dropout and college enrollment would change under different values of γ . Per Rosenbaum (2002), the bounds on the odds ratio that either of the two matched individuals will receive treatment is given by:

$$\frac{1}{e^\gamma} \leq \frac{P_i(1-P_j)}{P_j(1-P_i)} \leq e^\gamma,$$

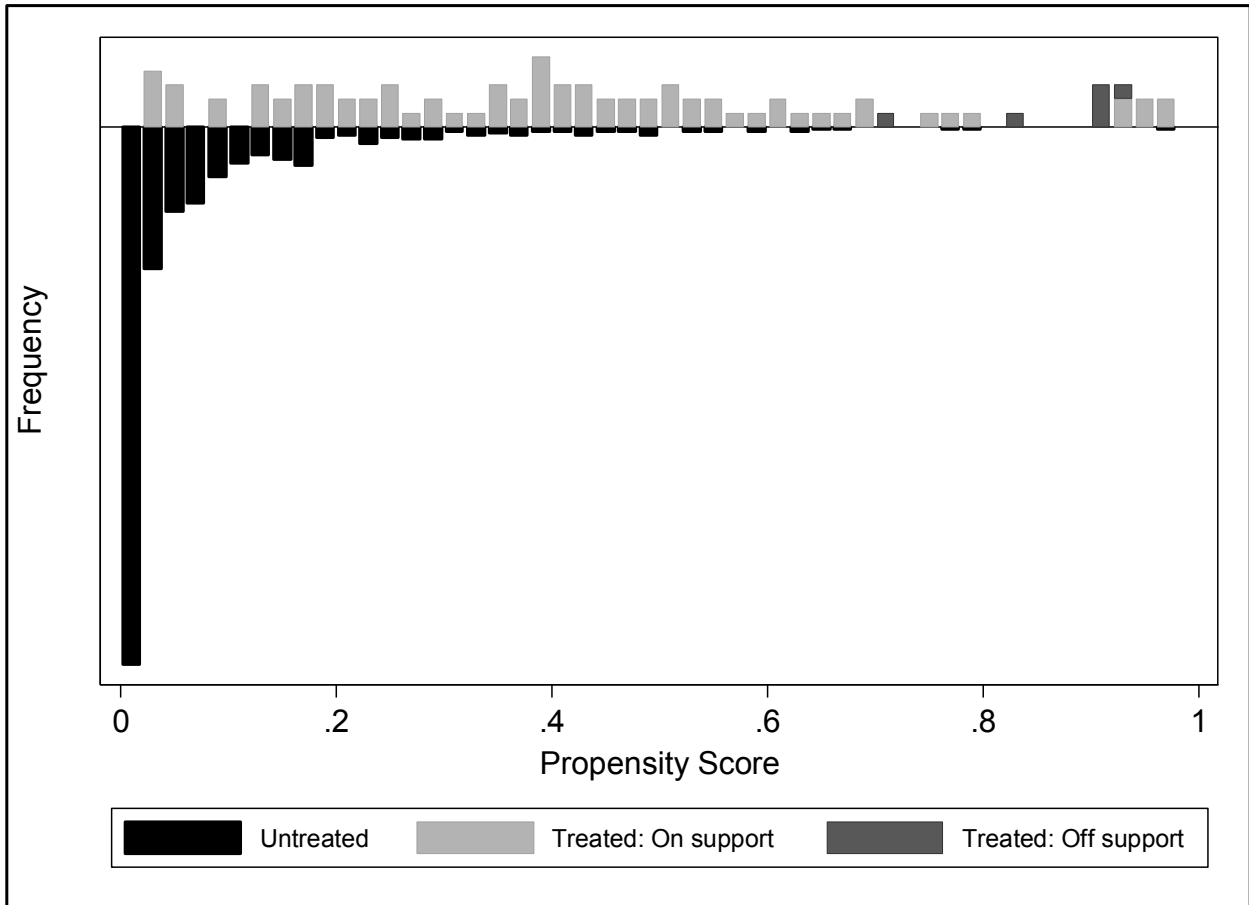
where $\Gamma = \exp(\gamma)$. Use of this bounding approach is suitable if matched pairs are mutually independent and pairwise matching is done without replacement (Becker and Caliendo 2007; Rosenbaum 2010:78).

We use the *mhbounds* routine in Stata to implement our sensitivity analysis, based on our results from one-to-one nearest neighbor matching without replacement. This command calculates Rosenbaum (2002) bounds for average treatment effects on the treated in the presence of hidden bias. The *mhbounds* command uses the Mantel and Haenszel (MH; 1959) test statistic, which is a non-parametric test that compares the observed number of arrested individuals who subsequently drop out (or enroll in college) to the expected number if the effect of arrest is zero.

The Q_{MH}^+ test-statistic adjusts the MH statistic downward in the event of positive unobserved selection, while the Q_{MH}^- statistic adjusts the MH statistic downward in the case of negative unobserved selection. The latter test-statistic represents the scenario where we have underestimated the treatment effect. For the former (Q_{MH}^+), positive selection occurs when arrested individuals are more likely to drop out of school for reasons other than their arrest. In this case, we would overestimate the treatment effect of arrest on dropout. The potential for such overestimation is our key concern.

Appendix Figure 1.

The Distribution of Propensity Scores, Cohorts 12 and 15,
By Treatment Status.



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Figure 1.

The Probability of High School Dropout and Enrolling in a 4-Year College Following Arrest, Individually Matched Arrested and Non-Arrested Youths.

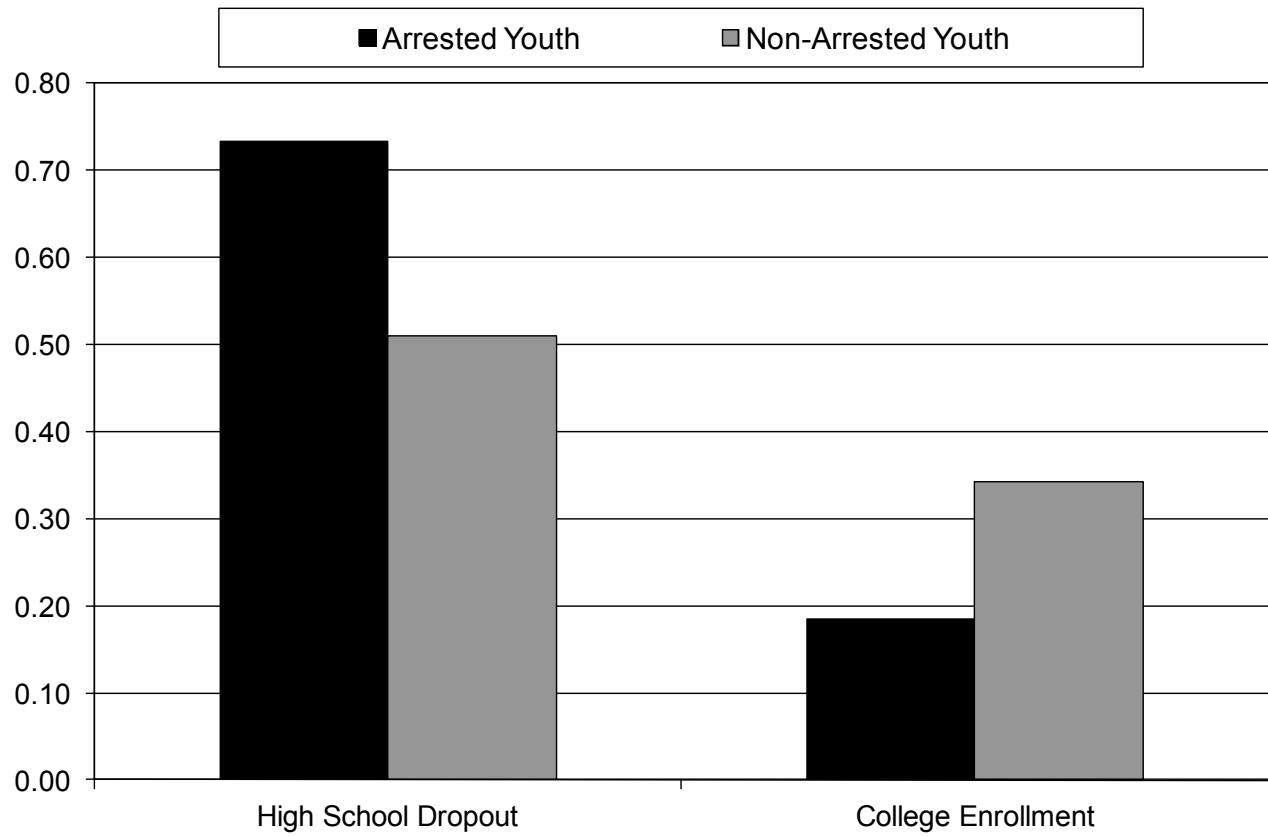


Table 1. Covariate Balance Before and After Matching, Individual-Level Characteristics, C12-15 Sample

	Means		Differences in Means		% Reduction in	Post-Match Hypothesis Test	
	Arrested	Non-Arrested	Unadjusted	Post-Match	Absolute Bias	T-Statistic	P-value
Youth Characteristics							
Male	0.71	0.41	0.30 ***	-0.02	92.9	0.91	0.363
Race-Ethnicity (versus Black)							
Mexican	0.18	0.32	-0.15 **	0.00	97.1	-0.44	0.663
Puerto Rican/Other Latino	0.08	0.13	-0.05	0.00	100.0	0.29	0.774
White	0.01	0.11	-0.09 **	0.01	91.1	-0.22	0.827
Other Race/Ethnicity	0.01	0.03	-0.02	0.00	100.0	-0.22	0.827
Cohort 12 (vs. 15)	0.54	0.51	0.04	0.08	-111.3	0.87	0.384
Age (Wave 1)	13.52	13.63	-0.11	-0.30	-174.9	-1.03	0.304
IQ	96.59	99.40	-2.80	-3.51	-25.2	-0.35	0.726
Student Mobility	2.79	2.61	0.18	-0.03	81.0	-0.33	0.741
Truancy	0.02	0.02	0.00	-0.01	-597.8	-0.31	0.756
Ever Retained in Grade	0.27	0.13	0.13 ***	0.08	42.4	0.54	0.592
Ever Special Education	0.49	0.25	0.23 ***	0.00	99.6	-0.84	0.404
Temperament							
Lack of Control	2.74	2.42	0.32 **	-0.03	89.0	0.27	0.789
Lack of Persistence	2.66	2.40	0.26 **	0.03	89.3	0.45	0.655
Decision Time	3.13	2.97	0.16	0.10	40.4	0.35	0.723
Sensation Seeking	2.94	2.74	0.20 *	0.04	79.5	0.53	0.600
Activity	3.70	3.59	0.11	0.01	87.6	-0.03	0.976
Emotionality	2.88	2.70	0.18	-0.01	92.9	0.42	0.671
Sociability	3.71	3.69	0.03	0.09	-240.2	-0.11	0.913
Shyness	2.41	2.47	-0.07	-0.06	10.8	-0.28	0.781
Problem Behavior							
Withdrawal	3.57	3.66	-0.09	0.14	-55.6	-0.80	0.425
Somatic Problems	3.90	4.07	-0.16	0.19	-15.9	-1.26	0.210
Anxiety/Depression	4.92	5.95	-1.03	-0.23	78.1	-1.34	0.182
Aggression	9.83	9.01	0.83	0.04	95.4	-0.72	0.475
Internalization	12.16	13.28	-1.12	0.14	87.5	-1.40	0.163
Externalization	14.06	12.53	1.53	-0.28	82.0	-1.05	0.297
Violent Offending	0.71	0.12	0.59 ***	-0.10	83.7	-1.18	0.241
Property Offending	0.23	0.07	0.16 *	-0.03	81.1	-0.33	0.741
Drug Distribution	0.21	-0.06	0.27 ***	-0.28	-2.0	-1.38	0.169
Marijuana Use	1.31	1.14	0.17	-0.04	74.5	-0.56	0.578

Notes: Data is drawn from Wave 1 of the PHDCN-LCS. *N* = 659.

* *p* < 0.05 ** *p* < 0.01 *** *p* < 0.001

Table 2. Covariate Balance Before and After Matching, Family and Peer Characteristics, C12-15 Sample

	Means		Differences in Means		% Reduction in	Post-Match Hypothesis Test	
	Arrested	Non-Arrested	Unadjusted	Post-Match	Absolute Bias	T-Statistic	P-value
Family Characteristics							
Immigrant Generation (versus Third)							
First	0.07	0.14	-0.06	0.01	87.0	0.20	0.838
Second	0.15	0.30	-0.15 **	0.00	100.0	0.00	1.000
Household Income	3.78	3.89	-0.11	-0.05	50.6	-0.17	0.862
Caregiver Occupational Status (SEI)	41.02	40.07	0.96	-1.08	-12.5	-0.39	0.694
Caregiver Education	3.01	2.87	0.14	-0.18	-24.7	-0.89	0.377
Married Parents	0.31	0.48	-0.17 **	-0.01	95.0	-0.12	0.908
Length of Residence	5.45	5.61	-0.16	0.35	-125.2	0.49	0.624
Extended Family in Household	0.28	0.20	0.08	-0.02	79.8	-0.24	0.814
Num. of Children in Household	3.73	3.41	0.32	-0.05	83.0	-0.15	0.878
Family Supervision	-0.07	-0.08	0.01	0.08	-471.1	0.71	0.480
Family Control	60.14	58.31	1.82	-1.66	9.0	-1.41	0.161
Family Conflict	49.45	47.77	1.68	-0.61	63.7	-0.35	0.723
Family Religiosity	61.81	60.80	1.01	0.82	18.9	0.83	0.406
Family Support	-0.10	-0.05	-0.05	-0.01	75.6	-0.10	0.924
Paternal Criminal Record	0.11	0.11	-0.01	-0.03	-324.5	-0.50	0.619
Paternal Substance Use	0.19	0.14	0.05	-0.03	36.9	-0.46	0.648
Maternal Substance Use	0.13	0.03	0.10 ***	0.01	91.2	0.16	0.872
Maternal Depression	0.15	0.17	-0.01	0.04	-194.9	0.75	0.453
Parent-Child Conflict	0.25	-0.08	0.33 ***	0.06	82.6	0.47	0.638
Home Environment							
Access to Reading	-0.26	-0.08	-0.19	-0.16	18.1	-0.49	0.621
Developmental Stimulation	-0.02	-0.07	0.05	0.06	-41.8	0.44	0.663
Parental Warmth	-0.14	-0.09	-0.05	-0.33	-563.2	-1.29	0.200
Hostility	0.42	0.53	-0.12	0.00	96.8	-0.01	0.996
Parental Verbal Ability	0.04	-0.01	0.05	-0.10	-87.6	-0.37	0.709
Family Outings	0.02	-0.14	0.16	-0.07	55.8	-0.58	0.562
Home Interior	-0.15	-0.18	0.03	-0.24	-712.4	-0.85	0.397
Home Exterior	-0.20	-0.09	-0.10	0.03	69.2	0.16	0.871
Peer Characteristics							
Friend Support	0.02	0.04	-0.02	0.00	-14.1	0.28	0.778
Peer Attachment	-0.10	0.03	-0.13	0.02	87.1	0.16	0.876
Peer School Attachment	0.12	0.04	0.08	-0.04	51.2	-0.58	0.566
Peer Pressure	0.20	0.08	0.11	-0.04	63.3	-0.23	0.821
Deviance of Peers	0.46	0.04	0.42 ***	-0.04	91.5	-0.26	0.793

Notes: Data is drawn from Wave 1 of the PHDCN-LCS. N = 659.

* p < 0.05 ** p < 0.01 *** p < 0.001

Table 3. Covariate Balance Before and After Matching, Neighborhood and School Characteristics, C12-15 Sample

	Means		Differences in Means		% Reduction in	Post-Match Hypothesis Test	
	Arrested	Non-Arrested	Unadjusted	Post-Match	Absolute Bias	T-Statistic	P-value
Neighborhood							
% African-American	54.89	36.80	18.08 ***	-0.46	97.4	-0.08	0.940
% Latino	25.66	32.08	-6.42	1.27	80.1	0.27	0.785
Concentrated Poverty	0.35	-0.06	0.41 ***	-0.04	89.3	-0.33	0.745
Concentrated Affluence	-0.33	-0.28	-0.05	0.01	78.6	0.11	0.909
Immigrant Concentration	0.12	0.38	-0.26 *	0.00	98.7	0.02	0.985
Residential Stability	-0.08	0.02	-0.10	0.07	27.6	0.42	0.674
Neighborhood Organizations	-0.28	-0.43	0.14 *	-0.02	85.4	-0.24	0.812
Neighborhood Youth Services	-1.65	-1.81	0.16	0.02	86.1	0.17	0.864
Legal Cynicism	2.54	2.52	0.02	0.01	74.1	0.30	0.768
Neighborhood Disorder	1.95	1.87	0.09 *	0.00	97.0	0.06	0.955
Tolerance of Deviance	4.21	4.24	-0.03	0.00	89.8	-0.13	0.896
Collective Efficacy	3.81	3.88	-0.07 **	0.01	85.2	0.30	0.764
Resident Victimization	0.44	0.42	0.01	-0.01	31.9	-0.27	0.785
LN(1995 Violent Crime Rate)	9.29	8.94	0.35 ***	-0.02	95.4	-0.18	0.854
School							
% African-American	65.72	48.20	17.52 ***	1.31	92.6	0.24	0.810
% Latino	25.42	36.03	-10.60 **	0.44	96.0	0.10	0.922
Enrollment	1462.64	1879.60	-416.96 ***	-16.00	96.1	-0.16	0.875
Poverty	79.54	76.74	2.80	2.33	20.9	0.99	0.324
School Mobility	59.29	31.04	28.24 **	2.41	91.8	0.12	0.905
% English Proficiency	9.55	12.27	-2.72	-0.06	97.9	-0.03	0.974

Notes: Data sources include the 1990 U.S. Census, the 1995 PHDCN Community Survey, the Chicago Police Department, and the CPS Office of Research, Evaluation and Accountability. *N* = 659.

* $p < 0.05$ ** $p < 0.01$ *** $p < 0.001$

Table 4. Rosenbaum Bounds, Effect of Arrest on Dropout

Γ	Q^+	p-value	Q^-	p-value
1.00	2.400	0.008	2.400	0.008
1.05	2.256	0.012	2.560	0.005
1.10	2.111	0.017	2.705	0.003
1.15	1.974	0.024	2.845	0.002
1.20	1.842	0.033	2.979	0.001
1.25	1.716	0.043	3.108	<.001
1.30	1.596	0.055	3.232	<.001
1.35	1.480	0.069	3.352	<.001
1.40	1.368	0.086	3.468	<.001
1.45	1.261	0.104	3.580	<.001
1.50	1.157	0.124	3.689	<.001

Notes: $N = 194$ (79 treated matched to 115 control youths).

Γ refers to the odds ratio of the effect of unobserved variables on the likelihood of arrest for youths who were arrested versus youths who were not arrested.

Table 5. Educational Expectations as a Mediator of the Effect of Arrest on Dropout

	DV: Educ. Expectations		DV: School Dropout			
	Model 1		Model 2		Model 3	
	Robust		Robust		Robust	
	Coef.	Std. Err.	Coef.	Std. Err.	Coef.	Std. Err.
Intercept	5.621	(0.073) ***	-0.950	(0.172) ***	1.535	(0.634) *
Arrested	-0.450	(0.344)	2.092	(0.550) ***	2.045	(0.621) ***
Propensity of Arrest	-1.215	(0.696)	3.998	(1.531) **	3.316	(1.478) *
Educational Expectations (wave 3)					-0.448	(0.111) ***

Notes: $N = 335$. Analyses of educational expectations are limited to the 12-year-old cohort.

* $p < 0.05$ ** $p < 0.01$ *** $p < 0.001$

Table 6. School Attachment as a Mediator of the Effect of Arrest on Dropout

	DV: School Attachment		DV: School Dropout			
	Model 1		Model 2		Model 3	
	Coef.	Robust Std. Err.	Coef.	Robust Std. Err.	Coef.	Robust Std. Err.
Intercept	3.126	(0.033) ***	-0.915	(0.171) ***	2.560	(1.004) *
Arrested	-0.154	(0.100)	2.065	(0.540) ***	1.978	(0.508) ***
Propensity of Arrest	-0.146	(0.286)	3.892	(1.503) **	3.532	(1.365) **
School Attachment (wave 3)					-1.122	(0.316) ***

Notes: $N = 335$. Analyses of school attachment are limited to the 12-year-old cohort.

* $p < 0.05$ ** $p < 0.01$ *** $p < 0.001$

Table 7. Friend Support as a Mediator of the Effect of Arrest on Dropout

	DV: Friend Support		DV: School Dropout			
	Model 1		Model 2		Model 3	
	Robust		Robust		Robust	
	Coef.	Std. Err.	Coef.	Std. Err.	Coef.	Std. Err.
Intercept	2.687	(0.018) ***	-0.834	(0.120) ***	2.185	(0.931) *
Arrested	-0.098	(0.052) +	1.454	(0.313) ***	1.398	(0.311) ***
Propensity of Arrest	-0.095	(0.098)	1.811	(0.693) **	1.758	(0.727) *
Friend Support (wave 3)					-1.132	(0.346) ***

Notes: $N = 659$. Analyses of friend support are based on the 12- and 15-year-old cohorts.

+ $p < 0.10$ * $p < 0.05$ ** $p < 0.01$ *** $p < 0.001$

Table 8. Rosenbaum Bounds, Effect of Arrest on College Enrollment

Γ	Q^+	p-value	Q^-	p-value
1.00	1.938	0.026	1.938	0.026
1.05	2.049	0.020	1.849	0.032
1.10	2.146	0.016	1.754	0.040
1.15	2.238	0.013	1.664	0.048
1.20	2.327	0.010	1.578	0.057
1.25	2.412	0.008	1.496	0.067
1.30	2.495	0.006	1.417	0.078
1.35	2.575	0.005	1.341	0.090
1.40	2.652	0.004	1.268	0.102
1.45	2.727	0.003	1.198	0.115
1.50	2.800	0.003	1.131	0.129

Notes: $N = 97$ (38 treated matched to 59 control youths).

Γ refers to the odds ratio of the effect of unobserved variables on the likelihood of arrest for youths who were arrested versus youths who were not arrested.